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CAN ASSET MARKETS BE MANIPULATED? A FIELD EXPERIMENT WITH RACETRACK BETTING

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SOCIAL SCIENCE WORKING PAPER 983

September 1996

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Abstract

To test whether naturally-occurring markets can be strategically manipulated, $500 bets were made at a large racetrack, then cancelled. The net effects of these costless bets gives clues about whether market participants react to information potentially contained in large bets. While the bets moved odds on "attack" horses visibly (compared to matched-pair control horses with similar pre-bet odds), the net effect on betting was close to zero. A second study with $1000 bets at a smaller track replicated the result. These markets could not be successfully manipulated, indicating that bettors did not mistakenly infer information from the experimental bets.

Keywords: Experimental Economics; Information aggregation; Field experiment; Market manipulation

JEL Classification Code: 051

*Wonderful research assistance was provided by Jessica San-San Tien, Dov and Amanda Rosenberg, and Hongjai Rhee. Excellent secretarial work was done by Gail Nash. Helpful comments were received from the MacArthur Foundation Preferences Group, the Russell Sage Foundation Behavioral Economics Summer Camp (Berkeley, 9/96), and discussions with Teck Ho, Hongjai Rhee, Charles Plott and Simon Wilkie. Preparation for this research was provided by Rich Palmer and Burt Camerer (who taught me to ask "why *is* that?").

I. Introduction

This paper reports an unusual field experiment which attempts to manipulate prices in a naturally-occurring asset market. The experiment is designed to address two questions: First, can markets be manipulated? Second, how well do prices in centralized asset markets reflect the aggregated information held by traders?

These questions have fascinated economists, and other social observers, for a long time. The answers are of practical importance because much of the case for the social value of organized asset markets-- particularly for derivative assets like options, stock indexes, etc.-- rests on the presumption that markets cannot be easily manipulated (so small investors are not bilked by large ones). While most economists probably do not believe market manipulation is widespread, many small investors do. The ability, or inability, to manipulate a naturally-occurring market in a controlled experiment provides rare evidence of the feasibility of manipulation.

A further claim is that information about important financial data is better conveyed if these derivative markets exist than if they don't, because prices aggregate information. It follows that if these markets don't aggregate information well, perhaps they shouldn't exist. The ability to manipulate the market provides indirect evidence about information aggregation.

Information aggregation

There is much more theory about aggregation of information than there is careful observation. The standard theoretical claim is that information could not be perfectly aggregated and revealed by prices because, if it was, no traders could profit from collecting information (the 'Grossman-Stiglitz paradox'). A corollary principle follows logically: If prices are a noisy reflection of what people collectively think assets are worth (e.g., due to exogenous shocks in the supply of shares or trading for liquidity), then the benefit to collecting information is restored, traders will pay to dig up information, and prices again have something to aggregate (but can do so only imperfectly).

Meanwhile, a decade or two of very careful, persistent experimental observation has proceeded (see., e.g., Sunder, 1995) in a one-sided dialogue with theory. In a typical experiment (e.g., Plott & Sunder 1982), traders are given free information about asset values. Typically traders have different information which is collectively perfect about the underlying asset value. When asset values take on two or three different values, and assets live just one period, information aggregates nicely so that the price which results from trading based on private information is close to the price which would result if everyone revealed their own information to others honestly. When information structures are more complicated-- e.g., two or three value-states in each of the two periods in which a two-period "bond" lives-- information aggregates less smoothly, due to persistent "mismatched" beliefs formed by traders, risk-aversion and "plunging" risk-preferences, and sometimes constraints on credit or short sales (e.g., Camerer et al, 1997).

Thus, the cumulation of twenty years of theorizing and a similar span of experimental observation is that nearly-perfect aggregation is a theoretical possibility which is commonly observed in simple experimental environments but rarely observed in complex ones.

Market manipulation

For present purposes, define market manipulation to mean trading against one's information, to create price movements which will lure other investors and ultimately allow later profit. Typical manipulations of this sort involve a firm's managers "propping" up its stock price by aggressive buying, then later selling as investors take the bait and prices rise.

If markets do not aggregate information perfectly, they might be vulnerable to 'market manipulation'. Since uninformed traders have no assurance that prices reflect aggregated information, they must try to infer what price movements are telling them. Then manipulators could make uninformative trades that might seem to convey information, but are actually made to deliberately mislead uninformed traders (for the purpose of later profit). There is a modest theoretical literature on this possibility but few data (e.g., Allen & Gale, 1992; Benabou & Laroque, 1992).

For example, in experimental trading periods in which no traders are informed, but uninformed traders don't realize that, uninformed traders sometimes overreact to trades which are similarly to previously-informative trades, creating "mirages" or falsely-revealing informational price bubbles (e.g., Sunder, 1992; Camerer & Weigelt, 1991). In these experiments, there is no evidence that traders are <u>deliberately</u> creating mirages. But since mirages do occur, and traders mistakenly become convinced during the mirage episodes that others have information (which was revealed by the prices), the possibility exists that extremely clever traders might deliberately mislead others. For example, Bill Gates could rush onto the floor of the NYSE and sell Microsoft shares hysterically-- causing astute traders to think Bill knew bad news, and sell-- while his minions buy up shares at depressed prices. While this possibility of market information manipulation is sometimes mentioned anecdotally[1], it is rarely observed in the wild, and hence, any such effects are hard to pin down and evaluate statistically.

An additional goal of this experiment is methodological. Field tests of information aggregation with naturally-occurring data are extremely rare, because the information held by traders is rarely observed. As a result, it is somewhat surprising that careful observation from controlled laboratory experiments has not influenced theorizing more. One reason may be that experimental markets are often dismissed as not representing 'real markets' (missing the point that such experiments test theories which are meant, presumably, to apply to either natural or artificial markets unless the domain of the theory's application is stated to be otherwise). This paper addresses this criticism by

[1] There is a well-known story about Nathan Rothschild, who was famous for having the fastest carrier pigeons in London, trading flamboyantly in the (ex post) wrong direction after receiving news about Napoleon in Waterloo, while his agents covertly bought up shares made cheap by Rothschild's ruse.

providing evidence from a true, controlled experiment on information aggregation behavior in a 'real', naturally-occurring market.

How this research came about

This research was inspired by two lucky observations.

First, I learned that attempts at market manipulation were observed in parimutuel betting experiments conducted at Caltech by Plott, Wit, and Yang (1996) (and earlier, by Nöth and Weber, 1996) In the Caltech experiments, subjects are given private information about which subset of events contains the true event. Different participants have different information (different subsets); the true event is always contained in the intersection of the subsets (sometimes uniquely). For example, if the events are labelled A-F, then some subjects know the true event (C) is A-C and others know it is C-E. Participants sometimes deliberately bet against their information, losing money by betting on events they knew with certainty would not occur. These traders were trying to manipulate the market: Since all traders knew that other traders might have different information than themselves, the would-be manipulators hoped to draw other bets toward their spurious choices, then later bet on the right events and collecting the money bet by the misled losers. While there is anecdotal evidence of this sort of manipulation in financial markets, its persistence in the laboratory raised afresh the question of whether such manipulation is possible in naturally-occurring markets (and particularly, in markets much larger than the laboratory ones).

Second, while using a computerized system at a large horse racing track, I mistakenly cancelled a "live" bet on a race not yet run. This mistake revealed the fact-- which is not widely publicized (and surprised most bettors I mentioned it to)-- that computerized bets on a race could be made, and then cancelled, before the race was actually run.

Putting these observations together, it became apparent that the attempts at market manipulation observed in the lab at Caltech could also be made, costlessly, at the racetrack. Since bets could be cancelled, the size of the bet one could make is only limited by technological constraints and time. Furthermore, the betting could be done in the form of a controlled experiment, with randomized assignments of the betting "treatment" to some horses, and a proper "control group" of horses that were not treated experimentally.

Does the potential information in these bets systematically mislead bettors-- as the participants in the Plott et al experiments tried to do-- or does the information aggregation process ignore my spurious (and costless) "speculative attack", much as a large wave ignores a small sand castle while sweeping it away? This paper answers that question.

II. Research Design

Facts about parimutuel betting

Before explaining the experimental design, some basic facts about parimutuel racetrack betting need to be explained. Throughout I talk only about "win" betting--

bettors collect money only if their horse wins the race, and lose their bet otherwise. Parimutuel betting means that bettors essentially bet against one another, rather than against a bookie who has designated odds. (Odds are the fraction or multiple of the bet if the event occurs. For example, 2-1 means bettors win \$2 for every \$1 they bet.)

In parimutuel betting, bettors may place bets any time until the race occurs. Suppose the totals bet on horse i at the time of the race are T_i and their sum is T. The track takes a percentage of the total bet, a cut which is predesignated and regulated by law and is usually 15-20% of the amount bet (for win bets). Then the remaining money is distributed to the winners in proportion to the total bet on the winning horse. For example, if horse 7 wins, those who bet on the 7-horse earn $(.85)T/T_7$ each, for each dollar they bet.[2] The "win odds", the fraction or multiple of earnings for each dollar bet, is $(.85T-T_7)/T_7$. Notice that the more is bet on a horse, the lower are the odds. The odds which matter are the final odds established by the cumulative total amounts bet when the race actually starts.

An important feature of this system is that, unlike betting with bookies at odds they fix, bettors who bet well before post time do not know the precise totals that will be bet on different horses by the time the race begins and hence, don't know the odds they will receive if their horse wins. Because of this uncertainty, it is hard to imagine why rational bettors would bet early, since they don't know the "price" they will be getting. Indeed, the cumulative total of bets does rises sharply toward post time.

In the racetracks at which we experimented, the totals of cumulated money bet are displayed on two large "tote boards" in the middle of the racetrack "infield", and also on computer screens throughout the track, every minute or so. (This fact is important, because it enables us to unobtrusively record a minute-by-minute time series of how betting totals are influenced by our own experimental bet, and bet withdrawal.) Figure 1 shows a time series of the mean amount of (cumulative) money bet across the races in our sample, relative to post time. Notice that about half the money is bet in the last three minutes before post time. This graph looks very much like the equivalent time series of cumulative betting in the Plott et al (1996) experiments, by the way, illustrating the potential parallelism between laboratory and natural markets.

Experimental Design

The basic design is simple. In each "attack" race we choose a close "matched pair" of horses with similar features.[3] A coin was flipped to determine which horse to "attack",

[2] Note that on a very heavy favorite, $.85T$ could be less than T_7, so that bettors get less back than they bet. In these situations (which rarely occur) the track is obliged to pay a minimum \$2.10 on a \$2 bet, losing money.

[3] The matched-pair design is often used when following control subjects is expensive or treatments are naturally scarce (as in some medical experiments with rare symptoms). Then randomized allocation of subjects to a control condition does not provide large enough samples to guarantee that uncontrolled differences between treatment and control subjects "cancel out", in the sense of the law of large numbers, so matching control and

or bet on; the other, unbet horse becomes a within-race control. The time at which a race is scheduled to begin is called "post time" (usually the race actually starts 1-4 minutes later, and bets are taken up to the moment that the starting gate opens and the race begins). The attack bets usually were made 17-22 minutes before post time (after about three minutes of betting had taken place), and the bet withdrawal came 5-8 minutes before post time. The bets took 30-90 seconds to make and cancel, so the effects of bets and cancellations sometimes appear in 1-2 minute intervals around the official bet/cancel times we recorded. We always bet $500 which, as we shall see, was enough to move the odds on the attack horses dramatically. (If this effect proves too small the bet size can be adjusted upward in further experiments.)

Criteria for experimental intervention

Data were collected from three tracks in Spring 1996. Races were chosen for experimental attacks if they had all of the following features:

(a). Two horses in the race have exactly the same "morning line odds"[4], from 8-1 to 50-1. We chose these relative longshots so that a $500 bet could move their odds dramatically. In 15 cases three horses were used (and in two cases four were used), with one serving as the attack horse and an average of the other two or three serving as a composite control horse. These composite controls smooth out variation in the control and improve test power.

(b). Within 1-4 minutes of the initial display of betting totals, and before the attack occurs, the two horses must have initial betting totals within 25% of one another (using the minimum of the two percentage discrepancies). Note that in many cases condition (a) was satisfied but (b) was not, since horses with similar morning-line odds often opened with very different amounts of money bet on them (and hence, different opening odds).

(c). An attack could be comfortably made around the window 19-23 minutes before post time, for "live races" (where bets were made at the track where the race was physically run), or 17-20 minutes before post time for "remote races" (where bets were made at one track on races run elsewhere).

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treatment subjects on as many potentially-relevant variables as possible may improve the ability to statistically detect the treatment effect.

[4] The morning line odds are a forecast by a handicapper employed by the track of what the post-time odds are likely to be. Matching on morning-line odds matches horses for any unobserved ability or characteristics (jockey, post position) thought by the track handicapper to influence the eventual odds. Note that since there is a mean downward drift in the percentage of money bet on both attack and control horses from start to post, and these horses are typically longshots, that implies the morning-line odds systematically underestimate actual post-time odds for longshots.

In the course of several months, gathering data carefully according to criteria (a-c), a total of 51 attacks were made and recorded.

III. Theory

Before turning to the results, it is worth working through some simple theoretical arguments about how these markets might react to our attempted manipulation. Parimutuel markets are interesting because traders could react to the amount that others bet in two different directions.

Informed traders and opinion traders

Suppose an informed trader has good, but not perfect, information that horse 3 will win. Then she might watch the tote board to see how 3's odds develop. If the odds are too low she may not want to bet. For this reason, if we attack horse 3, lowering its odds, betting from informed traders who might normally bet the horse might be pushed away by the attack. However, when our bet is cancelled the opposite effect should occur. Exactly the same pattern holds for an "opinion" trader who simply holds a firm opinion of a horse's chances, and is not swayed by the possibility that betting totals convey information.

Open-minded bettors

Now consider bettors who are uninformed (like opinion bettors) but react to information signaled by the betting of others. There is casual evidence that at least some bettors do react to price signals. For example, the Daily Racing Form, which contains detailed information on past performances of horses and advice for bettors, often says "watch the tote [odds] board" for information about horses who have not raced before, or not raced recently, or raced at a distant track.

Open-minded bettors will see that the attack horse's odds fall, implying that lots of bets have been made, and take that odds drop as a signal of good news about the horse. The attack bets will draw their money toward the attack horse. When the bet is cancelled, the odds rise, but generally not as much as they fell when the bet was made (because a larger pool of money is accumulating over time, so a $500 cancellation close to post time has less impact than the original early $500 bet). Hence, it is plausible that bettors watching the odds fluctuate would be drawn toward the attack horse initially, and not drawn away when the bet is cancelled.

These theoretical vignettes are no substitute for more formal theorizing about the betting markets, but no models exist which capture the institutional features of these markets, and constructing them is a challenging project beyond the scope of this paper. In addition, the predictions of such a theory would surely depend on the mixture of trader types presumed to exist at the track, which cannot be easily measured independently.

Thus, the reader should think of this sketch as simply a way to anticipate and understand what we might see in the data. The point is simple: If bettors simply ignore odds movements until nearly post time, or have information or strong opinions, then the attack bet will temporarily displace money from the attack horse; and the displacement may or may not be corrected by eager betting when the bet cancellation makes the odds rise. If bettors are uninformed and open-minded, reacting to information they think is conveyed by odds movements, the attack bet will temporarily draw money toward the attack horse. Of course, the most likely outcome is that bettors are some mixture of these types, so the interesting empirical question is whether there is any regularity in the tendency for money to be pushed away from, or drawn toward, the attack horse.

IV. Results

An event study look at the data

The initial analysis is a kind of "event study" popular in financial economics. In an event study, behavior of market movements around an event at time T_i (for event i) are aggregated across different events i. The method is simple: Subtract clock date T_i from event i, so the time series of events for event i is recoded -3, -2, -1, 0, +1, +2, +3... where 0 now corresponds to "event time" (formerly T_i). Then take statistical measures across events i at equally-coded event times. For example, if one wants to know the reactions at the time of the events, compare responses at time 0. If one wants to know whether people "anticipated" an event, study the negatively-numbered times -4, -3, -2, -1 leading up to event time 0. If one wants to know post-event reactions, study positively-numbered times, etc.

In this study, a bet or attack is an event. Time is broken up into discrete minutes (when the racetrack "refreshes" the parimutuel bet totals). There are three kinds of events: The "IN" time, when the bet is made; the "OUT" time, when the bet is cancelled; and "post time" when any net effect of both betting and cancelling are measured. Throughout, we report the effects at IN, OUT, and POST on attack horses relative to the same-race effects on control horses. Reporting relative comparisons controls very effectively for the mutual effects on both the attack and control horse of, say, unexpectedly heavy betting on a third horse in the same race, which would make the fractions of the total bet on the attack and control both fall.

Attack size

Figure 2 shows summary statistics. The top two lines show the time series of the median odds of the attack and control horses in minutes before post time. Denote the dollar totals bet on the attack horse, the control horse, and on all horses at time t by A(t), C(t), and T(t). The odds represent the fractions .85T(t)/A(t) and .85T(t)/C(t) across time. The bottom lines show the frequencies of times (relative to post time) that INs and OUTs occurred.[5] There is substantial variation in the IN times because bets were made at two different tracks, under different conditions.

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[5] The frequencies are actually divided by two to fit underneath the odds paths.

Median odds start around 20-1 for both horses, then dip to nearly 10-1 on the attack horses when the INs occur (17-22 minutes to post). The picture confirms that bettors watching the odds board for large movements will see one when the attack occurs. Since 20-1 odds imply that roughly 1/20 of the win pool is bet on a horse, and roughly $10,000 is bet on all horses when the attack bets are made, our $500 bet roughly doubles the amount of money bet on the attack horse, and cuts the odds in half.

Between minutes -16 to -7, while the attack bet is "live", the odds creep steadily up on the attack horse. This is the first clue that attacks do not draw a lot of money toward the horse. The upward creep suggests most racetrack bettors are informed or opinion bettors, not open-minded bettors who infer information from the attack bet and jump on the bandwagon.

When the bet is cancelled, usually within the -6 to -4 minute mark, the odds spike sharply upward. The odds on attack horses drift a bit further upward after that, but so do control-horse odds. Around post time, the odds on attack horses and matched control horses are about the same.

Figures 3-4 show the mean, plus and minus one standard error, of the time series of two statistics: The bet differential A(t)-C(t) in dollars (Figure 3) and the bet differential as a percentage of the win pool, (A(t)-C(t))/T(t) (Figure 4). Figure 3 simply confirms that the attack bets bump up the money on the attack horse by about $500. Both figures show that a little money is pushed away after the attack, between times -16 and -7, and the percentage differential shrinks (as lots of bets flow in, and the effect of the $500 differential shrinks in percentage terms). After the attack is pulled OUT, near post time, the dollar and percentage differentials are both very close to zero. (The upward spike in the second and third minute after post is caused by the large flow of money in the last couple of minutes of betting in Figure 3. It is not particularly significant, as indicated by the increased dispersion in the ± standard error bars.)

A further look at attack-control pairs

The event study time series show a large effect of the attack bets when the bet is made, but little net effect of these attacks. Another way to look at the data is to take each attack as a pair of observations. Each observation is a pair of numbers: (1) The first number is the net change in the percentage bet on the attack horse, measured from two minutes before the attack took place, at two minutes before IN, to post time (denoted A(post)/T(post) - A(IN-2)/T(IN-2)). The second observation (2) is the net change in percentage bet on the paired control horse, C(post)/T(post) - C(IN-2)/T(IN-2).

Keep in mind that our money is not on the horse at time IN-2, and the bet has been taken out by post time. Thus, the change in net percentage bet during this time reflects only any effect the attack has on other bettors, during the attack and between the time it is cancelled ~~and post time~~.

Figure 5 shows a scatter plot of the 51 attack-control pairs. Points below the identity line, to the right, indicate a more positive net effect on the attack horse than on the paired control horse; they represent cases where money is drawn by the attack. Points above the identity line, to the left, indicate the attack pushed money away.

There are about equally many points above and below the identity line. Figure 6 shows a histogram of the net effect at post time, (A(post)-C(post))/T(post). (This is just a cross-sectional slice of the event study time series in Figure 3 at post time.) The distribution is roughly normal, with a big spike around zero. The mean is .001172 and the standard deviation is .0186 ($t=.45$ testing the null hypothesis of zero mean). The median is -.00043 and 41% of the observations are positive ($z=1.09$ by a sign test). The studentized range (the range of data divided by the standard deviation) is 6.17, which is slightly fat-tailed compared to a normal distribution, because of the outlier at .08. No feature of the histogram reveals much evidence of systematic effects of attacks.

Returning to Figure 5, it is possible that half the attacks push money away and half draw money. If that were so, the mean effect on attacks would be no different than the change in control-horse bets-- explaining why the Figure 6 histogram is centered around zero-- but the variance of the attack horse changes would be large compared to control-horse variation. Looking at Figure 5 suggests this might be so, but keep in mind that some of the control horses are actually two- and three-horse composites. These composites are averages so they have less variance than single-horse controls. Adjusting the variance upward, the sample standard deviation for control-horse bet shifts (between IN-2 and post) is .01466, which is insignificantly smaller than the attack-horse standard deviation of .01699 by an F-test. The attacks do not change the mean amount bet, nor the variance in bet shifts, on the attack horses compared to matched controls.

Within- and post-attack bet shifts

While there is no detectable net effect of the attacks on betting, it is possible there is one effect while the attack is ongoing (between the IN and OUT points) and an opposite effect when the bet is cancelled, at time OUT.

To check for this, Figure 7 shows a scatter plot of attack and control bet shifts while the bet is live, between time OUT-1 (i.e., one minute before the bet is cancelled) and IN+1 (one minute after it was made). The plotted points represent the pairs [(A(OUT-1)/T(OUT-1)) - (A(IN+1)/T(IN+1)), (C(OUT-1)/T(OUT-1)) - (C(IN+1)/T(IN+1))]. Most points lie above the identity line, to the left, showing that after the attack the win pool percentage of the attack horse fell more than the control horse percentage (Figure 4 shows this too). Since the win pool is cumulating during this time, this means that the $500 bet is not matched by more money so it is shrinking in importance. Indeed, the event-study time series in Figure 3 suggests that the total dollar bet difference shrinks slightly-- a little bit of money is pushed away by the attack-- while Figure 4 shows a much larger shrinkage of the percentage-of-the-pool differential.

Figure 8 shows the attack and control changes between one minute after the bet is cancelled, OUT+1, and post time. The plotted points represent the pairs [(A(post)/T(post)) - (A(OUT-1)/T(OUT-1)), (C(post)/T(post)) - (C(OUT-1)/T(OUT-1))]. The points are evenly distributed above and below the identity line, indicating little systematic difference between control and attack horse after the bet is cancelled.

Cross-attack comparisons

It may be that attacks have effect on average, but do have effects that are predictably related to observables. To check for predictability we regressed the net attack effect (the difference in post minus IN-1 bet shifts between attack and control horses) against several variables: Track location; "attackability" of the race, measured by the total dollars bet on the attack horse at time IN; and duration of the attack in minutes. A weak positive effect of duration was found (t=1.39 when less significant variables were excluded). Further experiments could exploit this shred of predictability by varying duration more systematically than was done here.

A replication with larger bets at a smaller track

The initial sample shows no substantial net effect of the attack bets. To check the robustness of this result, a replication was conducted with 19 higher-impact bets. The replication was conducted at a smaller racetrack with about half the total betting volume of the three tracks studied in the first study. Several small changes were made in the design. In the light of the results of the first study, the changes were made with an eye toward giving manipulation its best chance to work.

A total of $1000 was bet, rather than only $500, in two separate $500 bets. Betting in two waves might fool people into thinking that more than one insider was betting the horse, causing open-minded bettors to take the bait and bet the attack horse.

Because this second location is smaller, it was possible to delay betting until approximately 13 and 9 minutes before post time. Many bettors think, probably correctly, that better-informed bettors bet later (in order to know more about what odds they will get, and to hide their information from the public as long as possible). Hence, by betting later we might be able to fool more open-minded bettors into thinking our bets revealed information.

In addition, because betting lines were short (often nonexistent), the bets could be reliably cancelled just before post time, at 3 and 1 minute before post on average. This too may enhance manipulation, by making the effect on the odds of cancelling the bet smaller, and giving bettors who were earlier drawn to the attack horse little time to change their minds.

The results of the 19 bets in this small-pool replication can be seen in Figure 9. The figure plots the geometric mean odds on attack and control horses in the 20 minutes before post time. As in the companion Figure 2, the lines at bottom show the frequency of times at which IN bets (solid line) and OUT cancellations (dotted line) took place. The initial attack (at around -13) drops the mean odds from 15-1 to 10-1 and the second attack drops the odds further to around 8-1. There is a small upward drift in the attack-horse odds in the gully between the last IN and the first OUT, from -7 to -4, which means bettors are not drawn sufficiently by the attack money to keep the odds low (as in Figure 7 above). After all the OUTs are made, around post time, the odds jump up a bit more but control-horse odds rise too.

Further analysis replicates all the results shown in the figures from the original 51 large-sample attacks. The mean changes between post+2 and IN-2 in the percentage bet on the attack horse is -.0148 with a standard deviation of .0128. The corresponding figures for control horses are -.0103 and .0202. (Adjusted for two-horse controls, the estimated standard deviation is .0244, which is curiously higher than for attack horses but not significantly so.) The mean attack-control bet shift is -.0045, with a standard deviation of .0269 (t=-.73 testing the null that the statistic is zero). The median is .007 and 11 of 19 observations are positive. Since these results are slightly on the opposite side of zero from the earlier small-bet results, pooling them gives evidence of no net effect that is even more powerful statistically.

One minor effect in the replication sample is notable. "Maiden" races are races for horses that have never won before (and often, are racing for the first time). Less is known about these horses, so bettors may watch odds movements more closely for clues about information, and manipulations may be easier. In the replication sample there were 10 maiden races and 9 races for winners. This unusually high percentage of maiden races permits a powerful test for a maiden-race effect. The net attack effect in maiden races is slightly positive (.00353) and marginally significantly larger than in nonmaiden races (-.01343, t=1.37, p=.09 one-tailed). The effect goes in the predicted direction: Attacks are more likely to draw money in maiden races.

V. Conclusion

The answer to my title question is "No". Parimutuel racetrack odds could not be systematically manipulated with a sample of 70 $500 or $1000 bets on randomly-chosen attack horses, compared with matched-pair control horses in the same race. There is no evidence during any period-- while the bet was "live", after it was cancelled, or over the entire pre-bet to post-bet period-- that other bettors responded systematically to the attacks. The bets also did not increase the variance of pre- and post-bet changes on the attack horse, relative to the controls, so it is not simply the case that attacks worked strongly in opposite directions.

One possibility is that these bets were too small, or the markets too large for the attacks to be noticeable. Larger bets might have different effects. More experiments could conceivably test that possibility, but the $1000 small-pool bets are already quite large. Those bets were 8% of the win pool, on average (and sometimes up to 15%).

Another possibility is that markets can be manipulated under some identifiable conditions. There are very weak effects of duration on attack shifts, and more positive effects in races on nonwinner "maiden" horses (when bettors might be trying to infer information from odds movements more closely). These features may provide clues about how further studies could be designed with a better chance of manipulating markets. (It is also quite possible the modest effects are statistical artifacts which will not replicate).

Perhaps the more plausible conclusion is that these markets simply aggregate information quite well and, accordingly, bettors know enough to ignore a large bet that is made far before post time and isn't backed up by a steady flow of money which keeps the heavily-bet horse's odds down. This is a blow to the beliefs of those who think markets are easily and routinely manipulated by large investors For those who do not believe manipulation is common, and instead are inclined to marvel at the mysterious intelligence of centralized markets populated by self-interested traders, the immunity of these markets to modest-scale attacks may represent something new to marvel at and explain.

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Figure 1 : Cumulative betting on all horses (mean across all races)

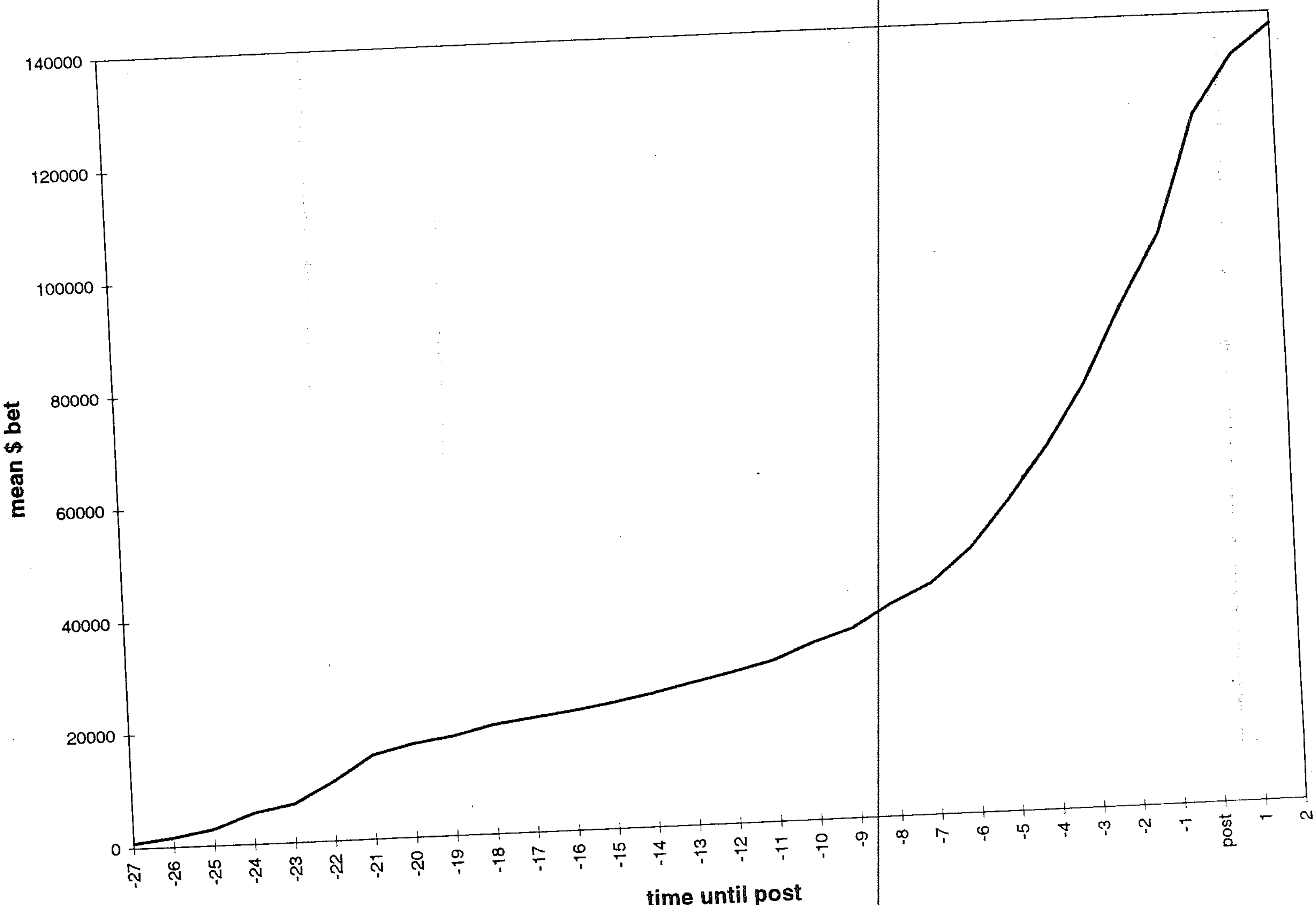

Figure 2 : Median odds on attack vs. control horses

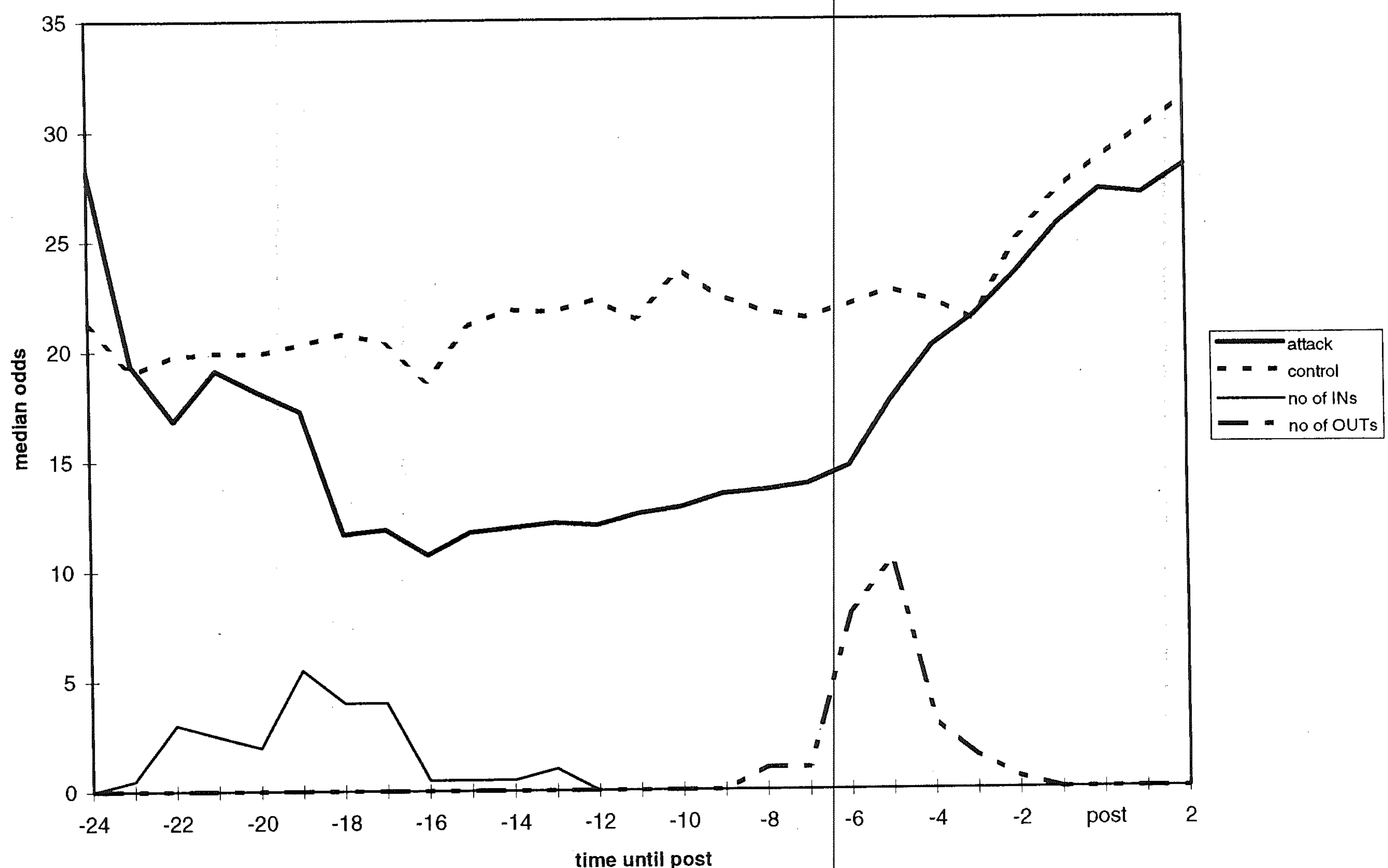

Figure 3: Mean +/- one standard error, Attack-Control in $

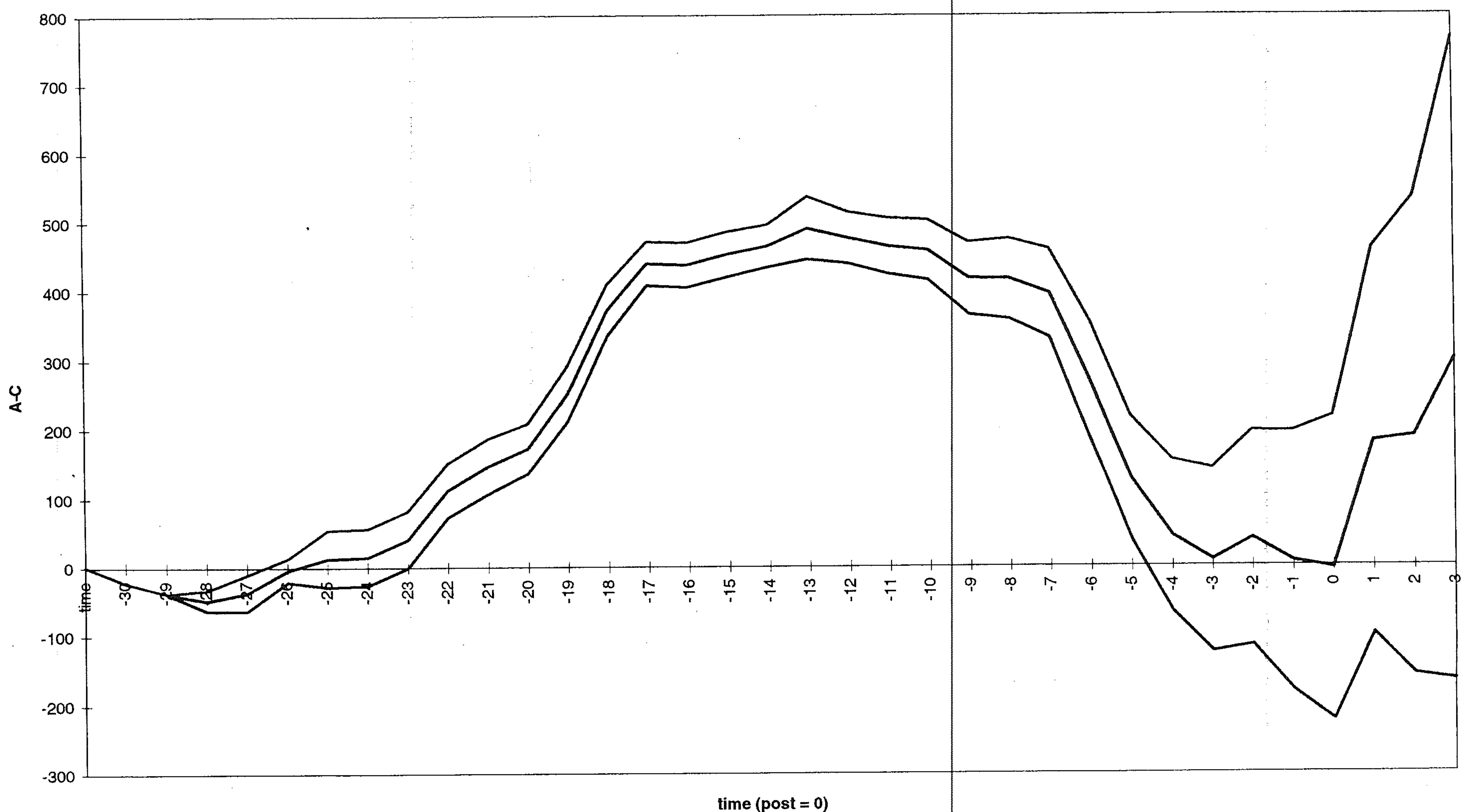

Figure 4: Mean +/- one standard error, Attack-Control as % of Total

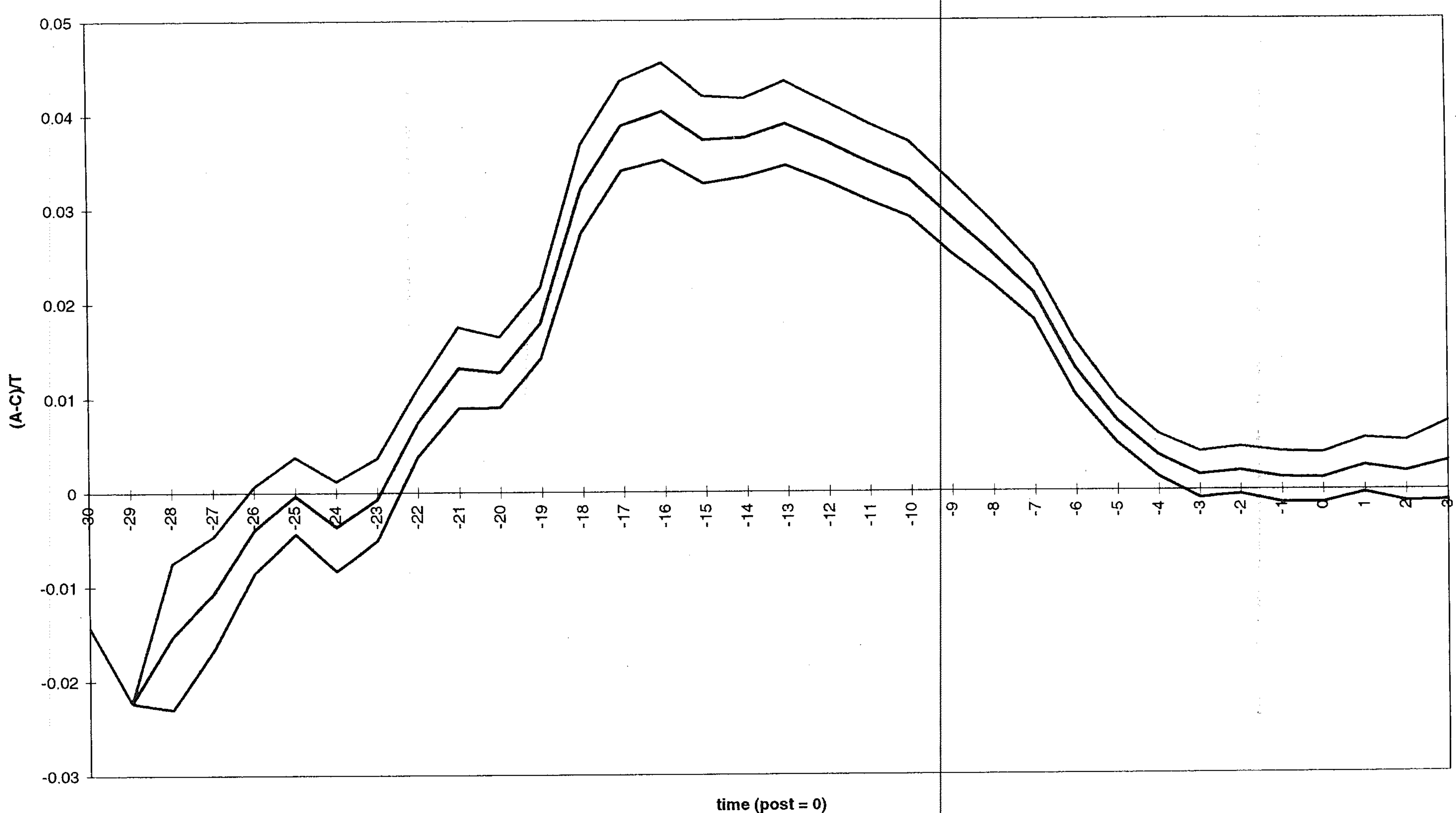

Figure 5: Attack vs. control bet shifts, post - (in - 2)

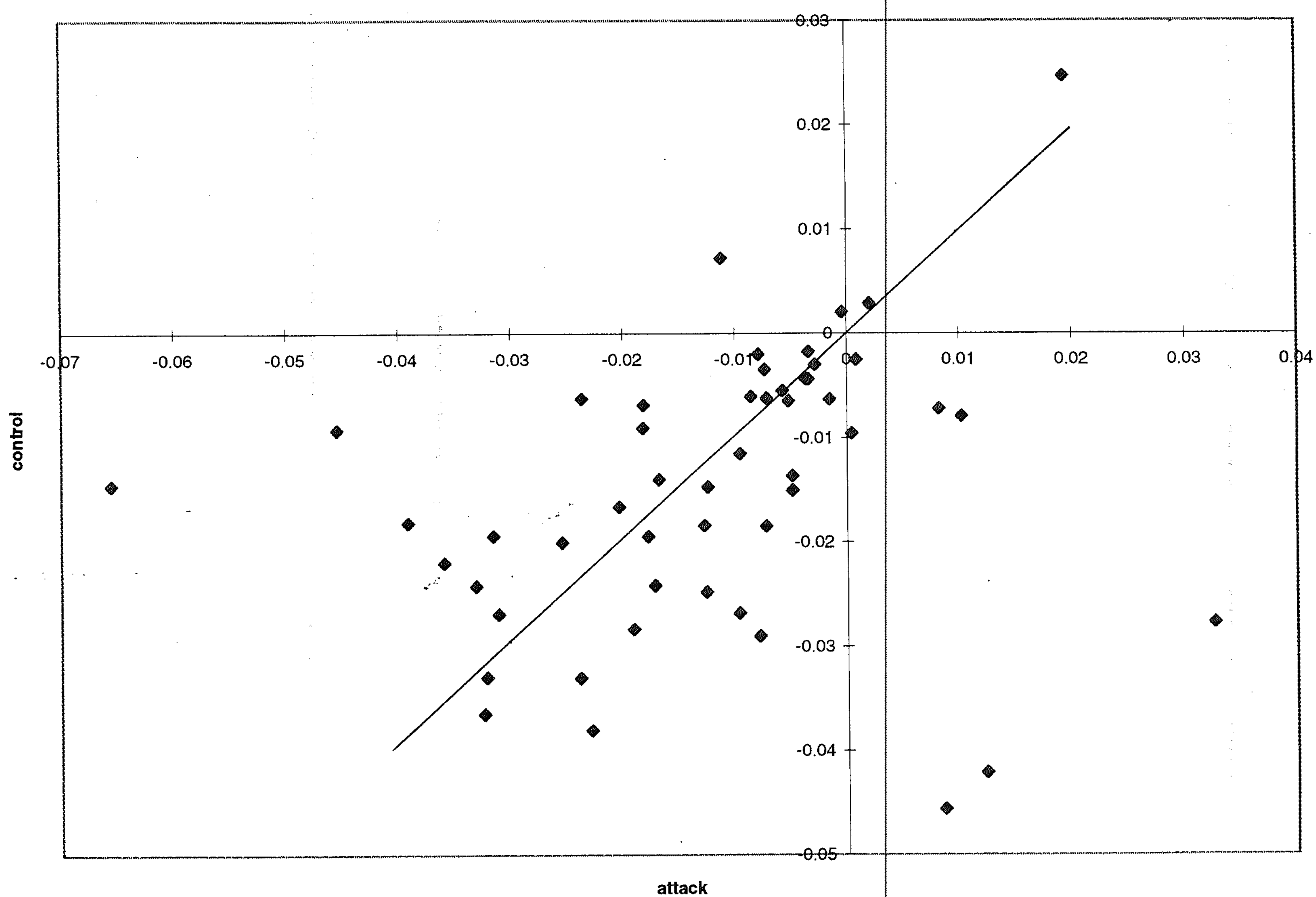

Figure 6: Distribution of net attack effect, (A-C)/T, at post time

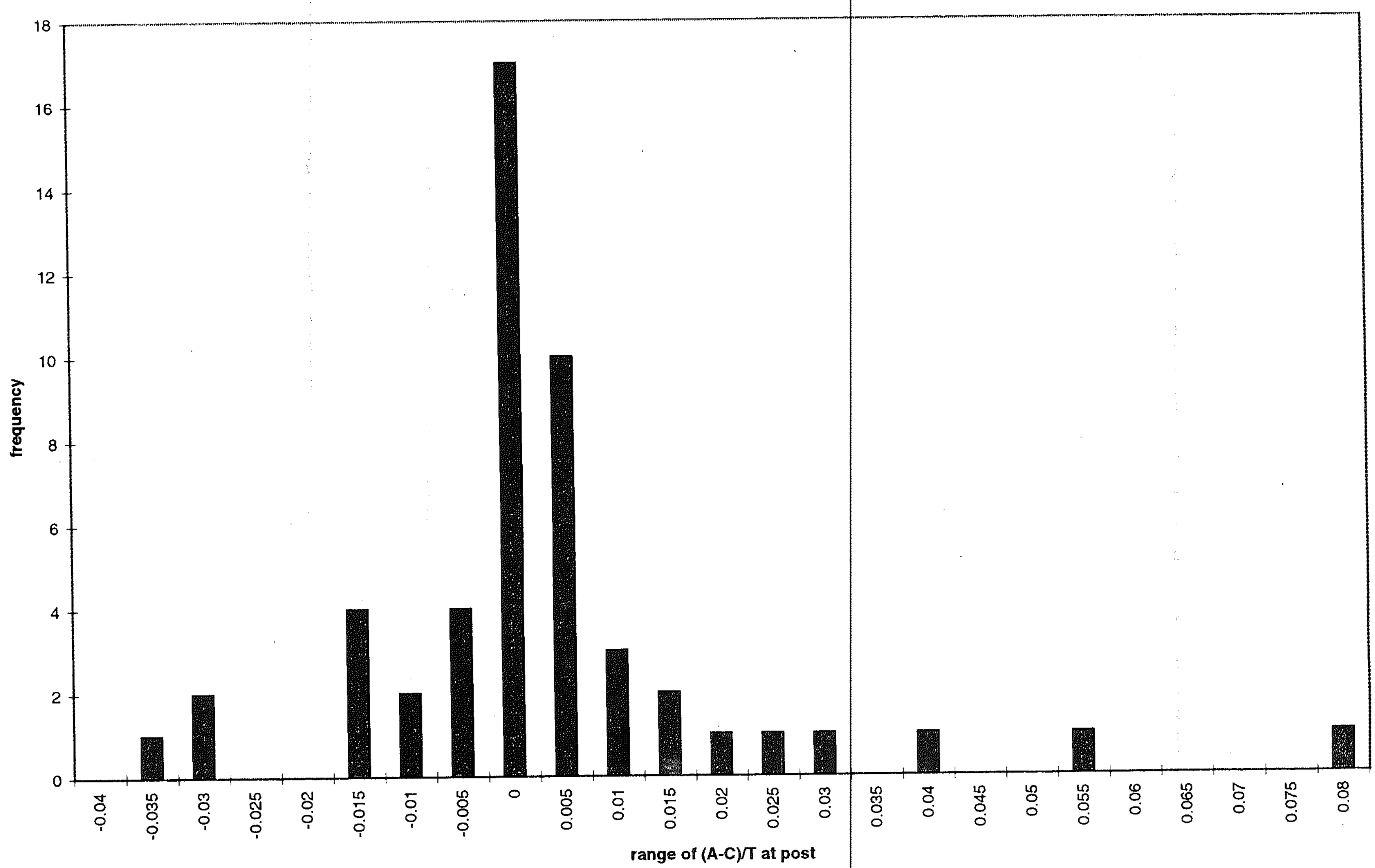

Figure 7:
Within-attack bet shifts, (out-1)-(in+1)

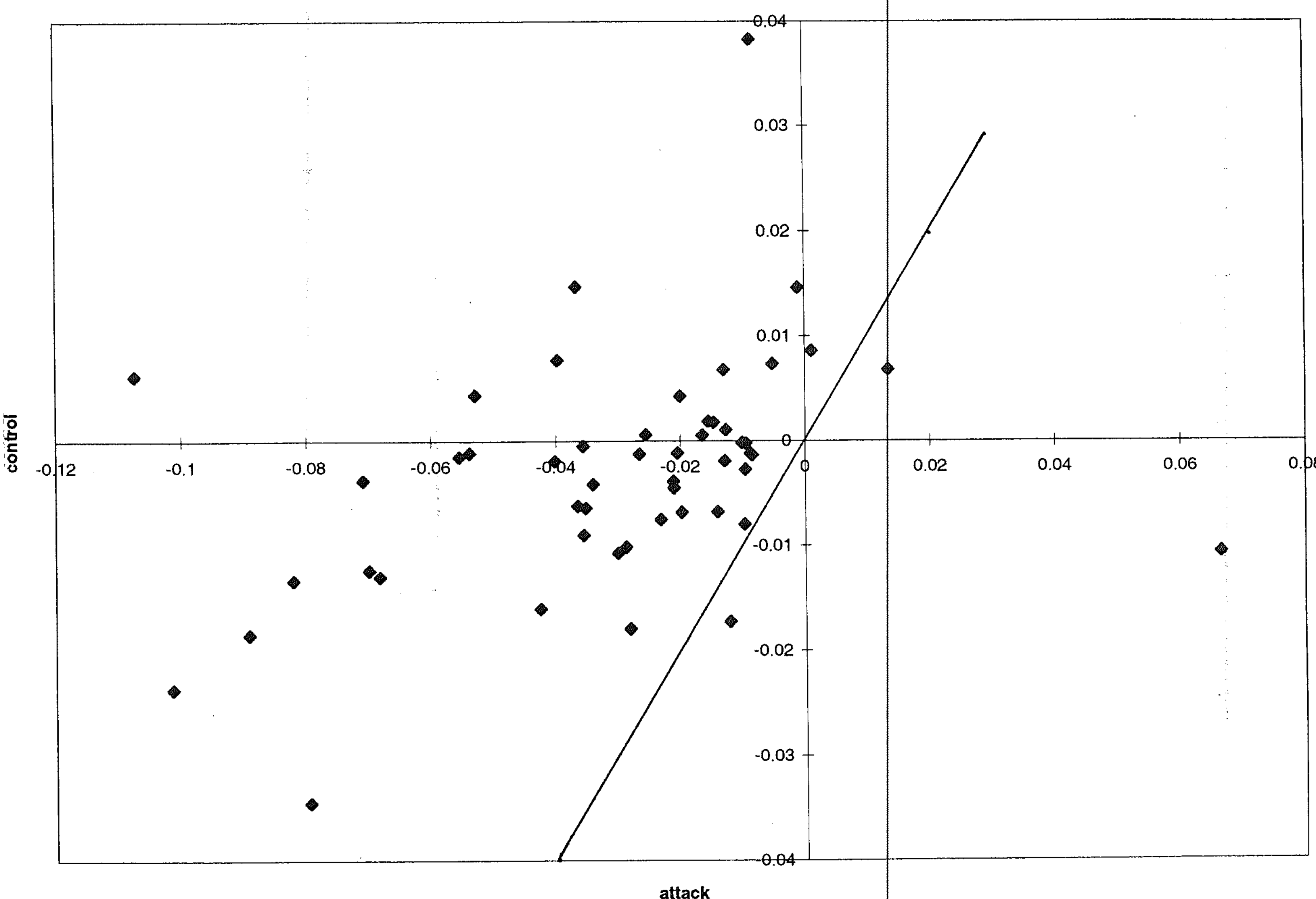

Figure 8: Post-attack bet shifts, post - (out+1)

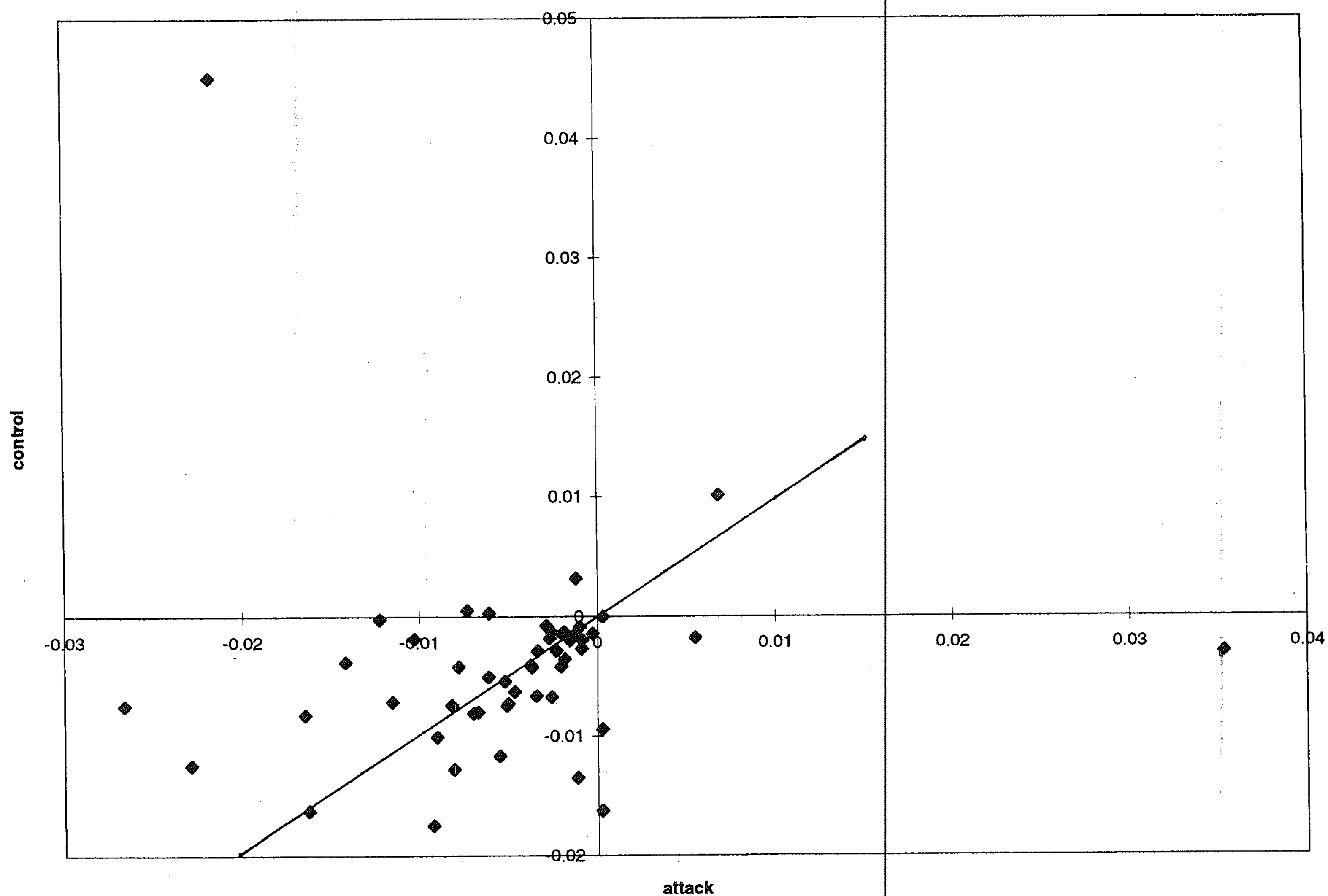

Figure 9: Geometric mean odds on attack vs. control horses, small-pool sample 2